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What's Old is New?

School Openings, Closings, and Student Achievement

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Abstract
In this paper we examine how student achievement changes after transfer to a newly opened school. Using data on all students and teachers in schools opening or closing in Michigan between 2010-11 and 2018-19, we estimate event study models of student outcomes as the local supply of schools changes over time. We find evidence for initial disruptive effects of transferring to a new school on math and English/Language Arts, which eventually are recovered over a three-year time period. Given the high cost of school construction and maintenance and the fact that students of color and lower income students are disproportionately among those attending an opened school, we discuss these results in the contexts of resources, equity and opportunity.[1]

Introduction

The impact of newly constructed schools on student and, or, community outcomes is an important though understudied policy question. What results when districts or other authorities open a new school (hereafter referred to as a "school opening") is relevant to several major and often controversial debates: the role that families' residential options play in their children's academic success; the role of school finance and of state and local funding options in equity and access; and—

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at times—opportunities for school choice within and between traditionally organized districts and across school sectors. In addition, school construction and access to high quality learning facilities are both questions of public investment. From 1995 to 2013, new building construction accounted for 45% of the $99 billion spent annually on school capital expenditures nationwide, despite overall public-school enrollment declines in 16 states (21st Century Fund, et al. 2016). Through 2017, the average age of an American school building was 44 years, with 12 years having elapsed since the last major renovation, on average (Education Week 2017).

Overall, 82 percent of capital outlays on school buildings are borne by local communities (Education Week 2017) leaving school facilities and maintenance, as well as access to state-of-the-art learning environments, sources of potential disparities between wealthy communities and those with poor economic bases (21st Century Fund, et al. 2016). Further, charter schools, which predominantly serve low-income students and students of color and account for one in three school openings in recent years, cannot levy property taxes and do not have access to any local revenues in over half of the states. Thirty percent of states do not provide any direct funding for charter school facilities (Rafa, Erin, Kelly, & Wixom, 2020). Thus, these disparities in access to facilities may contribute to educational differences between different populations of students, particularly since the underlying conditions of schools serving low-income students and students of color are poor relative to more advantaged peers (Filardo, et al. 2006; Alexander and Lewis 2014).

From the standpoint of public opinion, however, the evidence suggests general skepticism for new construction projects—particularly when property tax rates are substantially affected and voters are aware of those tax implications (Brunner, Robbins and Simonsen 2018, 2020). In the 1960s, local school bonds for new buildings passed at rates near 75%, dropping to only 35% at the century's end (Wirt and Kirst 1997). In Michigan, where the present paper is based, only half of school bond elections passed over the last two decades, with wealthier districts more likely to

support passage (Wilkinson 2016). In California, a 2020 statewide ballot initiative that would have funded $9 billion in costs for new facilities and improvement—including nearly $3 billion for new buildings—failed 53-47% (California Secretary of State 2020).

The high costs of building and maintaining new schools, the implications for equity, coupled with general public ambivalence for outlaying new taxes for new school openings (despite some apparent willingness to pay among wealthier communities), suggests that the implications of building new schools for the primary educational goal for any public school system—advancing student learning—should be better understood. Unfortunately, however, there are only a paucity of systematic studies of the effect of school openings to date—whether through newly financed construction or other policy-created initiatives—on student achievement. The studies that do exist tend to focus, for reasons of causal identification, on school bond impacts or similar interventions, and these papers—while technically sound from the standpoint of internal validity—limit inference to the impact of openings created explicitly by new bonding revenues.

In this paper, we add to this developing literature by shedding new light on the impact of opening new schools on student achievement in Michigan over the past decade. Using the universe of school and student data for Michigan students and teachers, we estimate dynamic event study models that include openings impacts on student achievement for both traditional public school (TPS) and charter school students, providing new evidence on the effects of attending newly opened charter schools in a mature education market. We also isolate the new school effect from what we call the disruption effect of switching schools found in prior studies, by using other students who switch schools as our comparison group. Doing so, we examine whether the effects of school openings differ when there is an established school culture and curriculum, as measured by the percent of teacher familiar with each other to isolate the effects of a newly constructed building from a "new" educational context. Finally, because we also observe the high volume of school

closings during this time period, we are able to situate student movement into new buildings within the larger context of dynamic school supply.

We find fairly consistent evidence of a negative impact of school openings on student outcomes in early years after transfer. However, we also show that students do regain ground in later years following their transfers which indicates that medium-run impacts of school openings may tend toward modest, if any, overall effects on achievement over several years. Charter school students who attend new schools recover more quickly from the initial achievement losses than students switching to new traditional public schools. Because we use other students who switch schools as the comparison group, these findings suggest that there is additional penalty to attending new schools, at least in the short term, beyond the disruptive effect of moving shown in more general student mobility papers. Also, we show that the recovery from these short-term learning declines is somewhat faster when students attend new buildings with a faculty that is somewhat familiar with each other, suggesting that longer-term negative effects may be driven by a newly formed school culture rather than a new building. In what follows, we discuss the limited literature on openings, our data and analytical strategies, summarize the results of our analyses and discuss the implications for education and policy.

Background: School Openings and Student Achievement

Only a handful of studies have directly considered evidence of the impact of school openings on student outcomes. Most of these studies leveraged new school choice policies within the traditional public sector. Hashim, Strunk and Marsh (2018) used a comparative interrupted time series to examine the effects of strategic openings to relieve overcrowding in Los Angeles, finding short term losses to student achievement as a result, which recovered eventually in subsequent years. Neilson and Zimmerman (2014) leveraged data on neighborhood school openings to estimate the effects of new school construction on test scores, public enrollment and property values for families living

near new buildings relative to those who lived in areas that would experience a new construction in the future. That study indicated increases in public school enrollment and property values, a positive effect on reading outcomes but null results for math as a result of school openings. Similarly, Goncalves (2015) found that Ohio's school renovation program increased enrollment and property values but saw negative impacts on test scores that improved with time while not eliminating achievement gaps. On the other hand, a study of the effects of school openings in Philadelphia on community outcomes such as crime rates, property values and overall public school enrollment found modest declines in property or violent crime associated with traditional or charter school openings, respectively (MacDonald, Nicosia and Ukert 2017) Two studies found that new small high schools of choice in New York City substantially improved graduation rates for those who attended, while not imposing additional average costs per graduate (Bifulco, Unterman and Bloom, 2014; Schwartz, Stiefel, and Wiswall, 2013).

In addition, a number of studies have utilized the results of policy change—whether at the state level or local bond referenda—that increased school facilities spending through capital expenditures. to identify school opening or facilities improvement effects. A recent study using Michigan data found delayed effects capital spending on student outcomes such as reductions in crime later in life (Baron, Hyman and Vasquez 2024), while an earlier paper from the same state showed school construction improvements can directly improve student achievement (Hong and Zimmer 2016). Conlin and Thompson (2017) found that students in Ohio school districts investing in new buildings suffered immediate short-term learning losses that were offset in future years once facilities construction was complete. Also in Ohio, Enami, Alm and Aranda (2021) showed increases in operating expenses had no average impact but did improve student achievement in high poverty areas—though a key driver was the inclusion of spending on teacher compensation as well. Cellini, Ferreira, and Rothstein (2010) leveraged near passage or failure results of school bond referenda to

estimate regression discontinuity effects of new school facility investments, finding such new investments improve home values while keeping demographic change relatively stable, but only modestly increase test scores. Similar studies by Hong (2017) and Martorell, Stange and McFarlin (2016) also leveraged bond results using a regression discontinuity design to show minimal impacts of new building expenditures on student outcomes, although there is some evidence from California that benefits may be delayed by six years or more (Rauscher 2020). A recent paper (Lafortune and Schonholzer, 2021) exploited differential timing of new school building construction to estimate the effects of attending a new facility, finding positive effects of attending a new building not only on student achievement after an initial post-move decline, but also on graduation rates and ultimately local housing prices. Finally, a meta-analysis by Jackson and Mackevicius (2024) included capital spending in its review, showing that average positive school spending impacts (particularly among historically disadvantaged students) are similar regardless of whether they are capital or non-capital outlays.

The Special Case of Charter School Openings

The demands imposed by identification of causal openings effects (e.g. leveraging the regression discontinuity-based estimates of school facility construction) has tended to provide evidence for broad patterns of new building creation within a traditional public school district. Apart from these papers, the prevalence of charter schools has increased steadily over the past two decades—in most cases resulting in the creation of new school. In fact, although only 8 percent of schools in the United States are charter schools, they make up nearly one-third of school openings. The corresponding literature provides some evidence of opening impacts in that sector. Several studies (e.g. Glomm, Lo and Harris 2005; Burdick-Wills, Keels and Schuble 2013; Koller and Welsh 2017; Saultz, Fitzpatrick and Jacobsen 2015) consider evidence on where charters choose to locate when they open. These papers generally indicate that charter schools open in racially diverse

districts—often near traditional public schools themselves—and often in districts with lower existing student test scores. However, little evidence exists concerning the effects of new charter school openings on student outcomes.

Disruption and Assimilation Effects

Taken as a whole, the literature concerning school openings finds that students who attend new traditional public schools experience either small increases in achievement or initial losses that develop into larger gains after multiple years. Studies have attributed early decreases in achievement either to the disruptive effects of switching schools or the high start-up costs of creating new school practices and culture (e.g. Hashim, Strunk and Marsh, 2018; Lafortune and Schonholzer, 2021). However, they have been unable separate the effect of a new school building on achievement from disruption effects or the impact of new school practices. Below, we discuss how these effects interact with the new building effect and how we attempt to disentangle them.

Other than kindergarteners, all students who attend a new school in its first year of operation switch schools. It is well documented that switching schools by choice or as a consequence of a school closure results in short-term achievement losses (Brummet, 2014; Engberg, Gill, Zammaro, and Zimmer, 2012; Hanushek, Kain and Rivkin, 2004; Ozek, Hanson, and Gonzalez, 2012). These studies also find that these effects are mitigated when the disruption results in students attending more effective schools and schools with higher levels of achievement (Brummet, 2014; Bross et al., 2023; Carlson & Lavertu, 2016; de la Torre & Gwynne, 2009; Engberg et al., 2012; Hanushek, Kain and Rivkin, 2004; Steinberg & MacDonald, 2019). Unlike studies of school closures, the disruption effect cannot be distinguished from the effects of the particular qualities of the school by using students attending the same school as the comparison group in studies of school openings because all students at the new school are treated. To overcome this limitation, we use other students who switched schools as the comparison group. Therefore, all

students in our sample experience a disruption, and any differential changes in achievement can be attributed to the qualities of the new school.

Students are not the only ones in a new schooling environment when a school opens, their teachers are in a new building as well. In most cases, the school leadership and faculty of a newly opened school must develop a new culture, curriculum, or operating procedures, which has high start-up costs. Because teachers tend to be the single most important school-based input to education production (Aaronson, Barrow, and Sander 2007; Chetty, Friedman, and Rockoff 2014; Hanushek 2011; Nye, Konstantopoulos, and Hedges 2004; Rivkin, Hanushek, and Kain 2005) it is likely that students moving to a new building with an established culture may be differently affected than those attending new buildings with faculty that are assimilating to new curriculum and operating procedures. Isolating these differences in what constitutes a "new" educational context is important to isolating effects of a newly constructed facility over other more education-specific factors such as teaching or curriculum. Thus, we hypothesize that new schools with a faculty that has already worked together are more likely to have a positive effect on achievement. We test this hypothesis by examining whether school opening effects vary by the proportion of faculty that worked with each other the prior year.

The Michigan Context

In this paper, we add to this collective literature by focusing on the effects of new school openings on student outcomes in Michigan. Michigan provides an ideal context to examine the dynamic effects of school openings because of the large number of school openings and closings in the last decade across the state in different geographic areas, including charter schools. Compared to national rates of school openings and closings, Michigan had a similar percent of schools open and a higher rate of school closures (National Center for Education Statistics 2023). Of the over 4,000

schools that operated in Michigan during our panel, approximately 6 percent opened and 13 percent closed.

Figure 1 displays the location of the 228 K-12 school openings that occurred in Michigan between 2009-10 and 2018-19. Over half of new school openings were charter schools. Both traditional (TPS) and charter schools opened throughout the state with nearly half of Michigan's school openings occurring in the Metro Detroit area. Figure 1 also displays the locations of school closures that happened during the panel. Although twice as many schools closed than opened between 2009 and 2018, many of the newly opened schools are located near where a school closed during the panel suggesting that school openings and closings are dynamic behaviors.

Data

We construct a panel of the universe of students and teachers in Michigan and estimate dynamic event study models across that population to determine the effects of newly opened schools on student math and ELA achievement. Specifically, we use student-level enrollment and achievement records for all Michigan public school students from 2009-10 to 2018-19 maintained by the Michigan Education Data Center (MEDC). These data include student demographic information and student test scores on state standardized achievement exams (either the Michigan Educational Assessment Program, MEAP, or the Michigan Student Test of Educational Progress, M-STEP). To determine when schools open, their location, and their terminal grades, we use a school-level dataset made publicly available by Michigan Department of Education (MDE) and the Center for Educational Performance and Information (CEPI) that includes the school's address, school opening and closing dates, their educational settings, and the grades they offer for all Michigan traditional public schools and charter schools that operated between the 2005-06 and the 2018-19 school years.

Outcome and Treatment Measures

We examine the effects of attending a school that newly opened between the 2010-11 and 2018-19 school years for students who attended it in its first year of operation on their achievement. We measure achievement using math and ELA test scores on the state standardized exam, the Michigan Educational Assessment Program, MEAP, or the Michigan Student Test of Educational Progress, M-STEP, for students in grades 3 through 8. We standardize the test scores within each grade, subject, and year. We note that the use of standardized test scores limits our sample to students in grades 3 through 8, thus we only examine the effects of school openings that affect those grades.

We define a school to be a new school in year *t* if it meets both of the following conditions. First, MDE and CEPI must consider the school as new, which we determine by a school's listed opening year found in the publicly available school data. New schools for a given year *t* have an opening date between May 31st of the previous school year to May of school year *t*. If a school changes its address or name, it retains the previous opening date for that school. Second, and in addition, the school must be located in a building that has not previously been used for educational purposes. Although we do not have data describing school facilities, we have a record of school addresses in the last 15 years. Specifically, a new school in our sample cannot be located at an address where a school operated in a prior year of our school-level panel, which may mean that a handful of charter schools operating in previously non-educational buildings such as old retail buildings could be captured in this definition (e.g., Imberman 2011). Such potential measurement error in a "new" opening aside (which in any case should attenuate our estimated results toward zero if an opening has a true effect in either direction), our final panel includes 34 traditional public schools and 73 charter schools that newly opened between 2010-11 and 2018-19 and served students who took the state exams during the first year of operation. To summarize, we consider a

school newly opened in *t* if it is in the first year of operation according to state records, and if there was no other educational entity at that building address in those state records since 2005.

We present the characteristics of TPS and charter school openings by year in Table 1, Panels A and B respectively. During our panel, traditional public schools opened across Michigan with nearly 60 percent of new schools located in suburban districts, especially in more recent years. The total enrollment and composition of the student body of newly opened TPSs varies from year to year. The differences in new school characteristics across years of openings likely reflect differences in student populations across locales. The characteristics of newly opened charter schools are more similar across years. New charter schools serve traditionally disadvantaged students and are located in urban and suburban districts. Most new charter schools in our panel opened before the 2014-15 school year.

To estimate the effect of attending a newly opened school, we use an event study design that compares changes in achievement of students who attend a school that opened in year *t*, to those of other students who switch schools between year *t-1* and year *t* but transferred to an existing building. We choose other students transferring schools as our comparison group because they experience a disruption to their schooling at the same time as students who attend new schools—an issue we discuss further below.[2] Thus, the estimated differences in achievement can be attributed to attending the new school itself, instead of the disruption effect attributed to changing schools. Our sample includes approximately 4,903,565 student-year observations representing about 1,026,216 students in grades 3 through 8 with either a valid math or ELA test score, who attend a school in a traditional public school district or a charter school that offers in-person instruction and general education, and

[2] For students in our comparison group who move multiple times during our panel, we use the year of their first move as the year they switch schools.

who changed schools at least once during our panel. We estimate our models separately by sector attended after the student switched schools to account for differences in sector effects.

Student and School Characteristics

We include indicators for the student's race, gender, English Learner, disability, and economically disadvantaged statuses as covariates in our models. Although we are unable to identify the individual reason every student transfers to a new school within our data, we are—in an improvement on much of the student school transfer literature—able to control for whether school transfers include a residential move as well. To account for a change in residence, which is directly tied to student mobility more generally (Hanushek, Kain and Rivkin 2004; Edwards and Cowen 2022), we also include a variable that equals one when a student moves to a residence in year *t* that is in the boundaries of a different traditional public school district that their residence in year *t-1*. To generate our school-level covariates, we aggregate student-level records. These school-level variables include the percent of students who are female, Black or Hispanic, economically disadvantaged, and English Learners as well as the percent of students with disabilities, and the logarithm of school's total enrollment. Additionally, we use the school's locale from the publicly available school-level data to create indicators for urban and rural schools.

We compare characteristics of students who attend newly opened traditional public schools and other students who switch to all other traditional public schools in the year prior to changing schools in the first two columns of Table 2. There are few demographic differences between students who attend newly opened schools and students who switch schools. Students who switch to newly opened TPS schools have slightly higher achievement prior to switching schools than other students who switch schools. However, three-fourths of students who attend newly opened schools in our TPS sample are non-structural movers, students who switch schools when they are not in the terminal grade of their previous school, compared to a quarter of switchers who do not attend a

newly opened school. Additionally, over two-fifths of students who attend a newly opened school attended a closed school in the year prior to the move, indicating the dynamics of school openings and closings. For these reasons, we estimate our models on a sample restricted to non-structural movers and a sample restricted to students who attended a closed school in the prior year as a specification check.[3] Results are similar and displayed in Appendix Figures A1 and A2. In the third and fourth columns of Table 2, we compare the characteristics of students who attend newly opened charter schools and students who move to existing charter schools. A lower percentage of students who attend newly opened charter schools are economically disadvantaged, Black, or Hispanic than students who move to an existing charter school. They also attended schools with a lower percentage of Black or Hispanic students prior to switching schools. Treated charter school students have higher average levels of achievement than charter school students in the comparison group. A similar percentage of students who attend newly opened charter schools are non-structural movers when compared to the comparison group. Few students in our charter sample attended a closed school prior to switching to a charter school.

Assimilation Measures

As we note above, a new school mostly made up of a staff from a previous school may not need to develop a new culture, curriculum, or operating procedures, mitigating some of the negative effects of attending a new school on achievement. Additionally, considering the extent to which a

[3] Students were included in our non-structural mover sample if they switched school between time t and t-1 (our main sample) and if they were not in the terminal grade of the school they were attending at t-1. Students in the terminal grade are not able to attend the school next year assuming normal grade progression. This means they were in the highest grade the school offers and the school did not add higher grades in the next school year. Students were included in our attending a closed school sample if they are in the main sample, and they attended a school at time t-1 in its last year of operation according to state data and has no other public school at the same address in future years of our panel. We define a school closure as a school with a closing date listed in our data that was between September 1st and August 31st of that school year (e.g., for 2009-10 school year the close date was between 9/1/09 and 8/31/10) and there is no other building with the same address in a future year of our panel.

faculty in a new building had taught together previously can help isolate a simple "new building" effect on outcomes because our measure of a school opening—while fairly detailed—remains an imperfect indicator of a phenomenon in which students and teachers essentially move from an old to a more modern facility. Thus, we test whether the effect of new school openings varies for schools with a faculty or student body that worked or attended school together the previous year.

We use teacher employment and assignment records for all Michigan public school teachers for the 2012-13 to 2018-19 school years to create our measure of teacher assimilation, which results in a restricted sample from the main analysis due to pre-2012 limitations in our teacher data. Specifically, we calculate the maximum percentage of teachers who teach at new school *v* in its first year of operation that taught at the same school*, u*, in the prior year to measure teacher assimilation. Equation 1 represents this calculation:

$$TeacherAssimilation_v = \max \frac{\sum T_{u,t-1}}{\sum T_{v,t}} \quad (1)$$

where $T_{u,t-1}$ indicates a unique teacher who taught at school *u* in year *t-1* and taught at school *v* in the year *t* and $\sum T_{v,t}$ represents the total number of teachers who teach at new school *v* in its first year of operation *t*. Therefore, schools with a teacher assimilation value of one have teachers who all taught together the previous year and schools with a teacher assimilation value of zero had no teachers who taught together the prior year. We display the average amount of teacher assimilation in newly opened schools in Table 1. On average, about 50% of teachers in newly opened TPS schools in our sample worked together the previous year. Only one-third of newly opened charter schools had over 50% of teachers working together the previous year.

Estimation Strategy

Determining treatment, as discussed above, brings us as close as Michigan's longitudinal student records allow to narrow our analysis to a focus on students actually enrolled in new school

buildings. What remains is the fundamental problem of estimating credible effects of those school openings given any bias created by shared variance in the incidence of school openings within a given area and in the determinants of student outcomes within that area. As we note above, 82 percent of the funding for recent school constructions was borne by local community outlays (Education Week 2017), with wealthier districts more likely to support new school building projects, including in Michigan, where this study is based (Wilkinson 2016). These patterns suggest that naïve estimates of mean differences between students in new schools compared to existing schools could overstate the impact of new school attendance if more advantaged students who live in communities with greater resources disproportionately attend newly opened schools. Even within school districts, students are not typically randomly assigned to new school buildings. Residential catchment areas—both existing and redrawn to reflect new school locations—could differentially affect different students depending on their family backgrounds and on where they live within the community. Therefore, we may expect systematic differences between students who attend new schools versus those who attend existing buildings both within and between districts.

In addition to non-random school opening location and non-random sorting of students into schools, there is also the empirical problem that the act of transferring to a newly opened school may pose. Earlier research has consistently found negative academic consequences for students who move between schools and students in classrooms with high rates of turnover (Alexander, Entwisle, & Dauber, 1996; Hanushek, Kain, & Rivkin, 2004; Ingersoll, Scamman, & Eckerling, 1989; Kerbow, Azcoitia, & Buell, 2003; Rumberger et al., 1999; South, Haynie, & Bose, 2007; Xu, Hannaway, & D'Souza, 2009; Goldhaber, et al. 2021), regardless of school quality or parental intentions (Hanushek, Kain and Rivkin 2004). If a non-structural transfer between schools (i.e. a move that occurs before a school's terminal grade served) is educationally disruptive, all else equal, we may expect students in new buildings to realize a decline in academic outcomes. This

might be especially true in cases where the entire learning environment changes, with new teaching staff and new school-grade peers, along with the school facility itself—a problem we consider below.

We exploit the large number of school openings that took place in Michigan between 2010-11 and 2018-19 and compare changes in achievement of students who attend a new school to students who switch to existing schools the same year to estimate the causal effect of attending a newly opened school using a difference-in-differences design. Recent research finds that difference-in-differences studies using two-way fixed effects with staggered treatment timing—in our case, different years of school openings—and treatment effect heterogeneity may produce biased estimates especially when previously treated units are used as comparison units (Baker, Larcker, & Wang, 2022; Goodman-Bacon, 2021; Callaway & Santa-Anna, 2021). Our research design, which removes the disruption effect that may occur through upon school transferring from the effect of attending a newly opened school, creates a unique comparison group for each year of school opening, allowing us to account for dynamic treatment effects for each year of treatment using an event study design. In particular, we compare outcomes for students who move to a newly opened school in year *t* to students who switch to existing schools between years *t-1* and year *t* creating a cohort of observations that become treated or stay untreated relative to year *t*, assuming that absent either form of transfer previous trends in outcomes would have remained the same. For example, students who attend a newly opened school in 2015-16 and students who switch to existing schools between the 2014-15 and 2015-16 school years are in the 2015 cohort, with the former being our treatment group in the 2015 cohort, and the latter being the comparison group in that 2015 cohort. This design ensures that there is a separate comparison group of never treated observations for each year of treatment. Therefore, the dynamic treatment effects of future-treated and previously treated observations do not bias our treatment estimates. In essence, we are estimating a statewide impact of

transferring to a newly built facility, separated from any impact of transferring to an existing school that is simply new to the student, with the latter group representing the never-treated cohort.

Thus, to estimate the effects of attending a newly opened school on achievement, we estimate the model represented by Equation 2: [4]

$$Y_{icst} = \beta_0 + \sum_{r \in R} \delta_r AttendsNewOpen_{ictr} + \boldsymbol{X_{it}\alpha} + \boldsymbol{S_{st}\rho} + \mu_i + \theta_{ct} + \varepsilon_{icst}, \quad (2)$$

where Y_{icst} is the math or ELA score of student *i* in move year cohort *c* who attends school *s* at time *t*. Our variables of interest, $AttendsNewOpen_{ictr}$ equal 1 if student *i* at time *t* in cohort *c* attended a new school that opened *r* years ago. Negative values of *r* represent years until the school opens.[5] $\boldsymbol{X_{it}}$ are time variant student characteristics that includes indicators for being economically disadvantaged, an English Learner, a student with a disability, a residential mover, or a structural mover. $\boldsymbol{S_{it}}$ are school characteristics including the percent of students attending the school at time *t* that are female, Black or Hispanic, economically disadvantaged, an English Learner, and a student with a disability as well as the natural logarithm for the school's total enrollment and indicators for attending a school located in an urban or rural locale or a charter school. μ_i is a student fixed effect that controls for time invariant student characteristics. θ_{ct} is a cohort by year fixed effect that ensures that we only make comparisons between students who change schools at the same time for each year before and after they switch schools. Standard errors are clustered at the student level. We estimate the effects of attending newly opened TPS and charter schools separately. Students in each sample switch to a school in the respective sector when *r=0* regardless of if they were attending that sector previously.

To examine how the effects of new school openings differ for new schools that have staff that are familiar with each other, we estimate a model that includes interactions of the treatment

[4] We also estimate our model without covariates. Results are similar.
[5] We restrict our sample to observations we observe within five years of treatment.

indicators with the teacher assimilation measure. We operationalize teacher assimilation as a binary indicator that equals one when more than 50 percent of teachers have taught together. Specifically, we estimate:

$$Y_{icst} = \beta_0 + \sum_{r \in R} \delta_r AttendsNewOpen_{ictr} * Assimilation > 50\%_{s,r=0} + \sum_{r \in R} \delta_r AttendsNewOpen_{ictr} + \boldsymbol{X_{it}\alpha} + \boldsymbol{S_{st}\rho} + \mu_i + \theta_{ct} + \varepsilon_{icst} \quad (3)$$

where $Assimilation > 50\%_{s,r=0}$ equals one when student *i* who attends newly opened school *s* in *r* years that has more than 50% of its teachers who taught with each other the previous year during the school's first year of operation. This measure is time-invariant because we hypothesize that new schools that have no teachers that are familiar with each other have a different effect on student outcomes than other school openings.

Results

We present the estimated effects of attending a newly opened school on math and ELA achievement in Figure 2. The results for the TPS sample are displayed in Panel A. The first major result is that students who attend a newly opened school experience a decrease both math and ELA achievement of approximately 0.03 standard deviations in the year the school opened.[6] These students experience a slightly larger decrease in the second year of operation. Because we are comparing students in a new building to those who also moved but moved to pre-existing buildings, these results could indicate a negative impact of moving to an entirely newly constituted educational environment that itself needs time to mature in ways that pre-existing schools receiving new students do not. These negative impacts of attending new schools do dissipate, though, and eventually reverse themselves in results that fit well with a pattern in Los Angeles seen by Lafortune

[6] We note that if we use r=-2 as the reference group instead of r=-1 in our models, the negative effect of attending a newly opened school on ELA achievement would not be statistically significant for the first year of operation. All other results are similar when the reference group is changed to account for the possibility that differential pre-trends could affect our results.

and Schonholzer (2021). There is an upward trend in achievement in the next three years after the move. In addition, we see that the effects of attending a newly opened charter schools are similar and are displayed in Figure 2 Panel B, despite the possibility that a charter opening may be different than an opening in a TPS district created, say, by local resources. Attending a new charter school only has a negative effect on student achievement in the year the school opened. By the third year after the move, students who attended a new charter school in its first year of operation experience increases in both math and ELA test scores.

One possible explanation for the only negative impact of attending a newly opened school in the first year or two of operation may be that it takes time for a teaching staff establish or re-establish a culture, curriculum, or operating procedures. As we describe above, previous literature on school mobility has shown that moving schools can have a negative impact on student outcomes even when the educational environment is thought to be more productive. Much of that specific academic disruption possibility is already accounted for in our method—as discussed above, one novel aspect of our approach is that our treatment of moving to a brand new school is estimated against a comparison group of students who also move in the same year, but to pre-existing schools instead, netting out a disruption effect due only to moving itself. Nonetheless, the possibility that students still receive a negative impact on their learning in a new building could plausibly be weaker, with student recovery taking less time if the building is new but the educational environment—at least insofar as it is represented by teachers—is largely transferred to a new building and staff are already familiar with each other. To test this, we estimate the model represented by Equation 3 and discussed above. We present the results of this model in Figure 3. In our TPS sample, the increases in achievement students who attended newly opened schools experienced 3-5 years after switching schools are concentrated in the students who attended schools with more than 50% teacher assimilation in the year of the school opened. In our charter school sample, the initial decline in the

first year of operation is concentrated in schools with 50% or less teacher assimilation. Overall, the estimates are presented net of the disruptive effect of moving to a new school in general (whether newly built or existing) and account for heterogeneity in students' previous exposure to teachers in their new school.

Discussion

In this paper we provide new evidence for the impact of school openings on student achievement. Using a rich panel of data on the universe of students and teachers in Michigan from 2009-10 to 2018-19, we estimate event study models that compare the impact on student achievement of moving to a newly built school relative to moving to an existing building. After an initial decline in Math and ELA in the first year after such a transfer, we find gains to student outcomes in later years after the move, such that the impact of transfer to a new facility appears minimal after both the immediate decline and eventual recovery are taken into account. This pattern of loss followed by recovery is consistent with recent evidence (e.g., Goncalves 2015; Hashim, Strunk and Marsh 2018; Lafortune and Schonholzer 2021), which show similar patterns of short - term disruption followed by achievement gains. Analytically our paper is similar to Lafortune and Schonholzer (2021), but our comparison group consists of movers to existing facilities in the same year as students moving to a newly opened school. Although our results are similar on the surface, our paper thus has the advantage of netting out such disruptive effect of moving to a different educational context in general and of the addition of statewide panel and charter schools to our data. We also consider whether students who moved to schools with teachers from their previous buildings realize weaker achievement declines after initial transfer, and show that this possibility is indeed the case.

Taken together, then, this paper's contribution is to focus specifically on the impact of new school construction on student outcomes, netting away the disruptive effect of transfer to a different

school building more generally, and allowing for the possibility that students who move to buildings where the teaching staff includes familiar faces from students' previous schools have different transfer impacts. Our findings are plausibly causal estimates of the impact of school openings on student achievement but include two important caveats. The first is any measurement error in what amounts to a school opening. As we explain, we define a "school opening" as a school in which the state's administrative code is unique to that school, and the school's address, are together new in a given year within the panel of data. We believe this is a straightforward definition of a new school in Michigan, but it is possible that this definition includes a handful of schools—especially charter schools—that may have reconstituted with a similar staff and educational mission into a building (e.g. Imberman 2011) that previously served in a non-educational function, thus excluded from our definition of a newly built school. This could potentially underestimate the results we find, but both TPS and charter results are similar here. The second caveat is that our dynamic event study approach must satisfy the assumption that students who moved to either new or existing buildings would have trended in comparable ways absent such transfers. We argue that our approach and its robustness checks provide reassurance that this assumption is valid, but cannot fully discount the possibility that new openings are non-randomly distributed to students in ways that discredit our approach.

Such caveats notwithstanding, these results have a number of implications. The first is that they provide new evidence of a pattern that exists in what limited literature is extant on school opening effects. Namely, that there are only modest discernible and systematic gains to student outcomes as a result of new school openings, and these tend to appear only in the years following students' move to a newly built facility. In the immediate aftermath of a transfer, on the other hand, achievement is disrupted, and we observe negative impacts that dissipate over time. That school openings are, at least in Michigan, drawing heavily on Black and economically disadvantaged students, implies that this pattern of short-term decline followed by modest gains in achievement

suggests that equity issues involving which students learn in new state-of-the-art facilities are less straightforward than concerns that students in older buildings are disadvantaged as a result. This is especially the case given the high cost of school facility construction and maintenance, which adds up to nearly $50 billion annually (21st Century et al, 2016). That new school openings consume huge portions of local resources, that those resources are inequitably distributed, and that there are other substantial local differences in schooling conditions apart from schools and classrooms raises the question of whether on average the costs outweigh the benefits when the best we can say about new school buildings is that over time students who transfer into them make up for short term learning loss. We do not consider other non-achievement indicators like property values, or less tangible outcomes like families' pride in neighborhoods and communities, or comfort or safety in physical learning conditions that may result from attending a newly opened school, but from the standpoint of math and reading outcomes, at least, the results here suggest that the impacts are mixed at best.

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TABLES AND SUMMARY DATA

Table 1. Characteristics of Newly Opened Schools by Sector and Year

Panel A: Traditional Public Schools									
	2010	2011	2012	2013	2014	2015	2016	2017	2018
Avg. Pct. Female	57%	51%	49%	54%	65%	47%	N/A	51%	49%
Avg. Pct. Black or Hispanic	11%	13%	29%	38%	100%	40%	N/A	14%	8%
Avg. Pct. Econ. Dis.	48%	31%	62%	64%	91%	56%	N/A	41%	36%
Avg. Pct English Learner	1%	0%	19%	0%	0%	2%	N/A	4%	0%
Avg. Pct SWDs	32%	2%	10%	6%	0%	12%	N/A	18%	9%
Avg. Enrollment	435	126	344	54	23	328	N/A	519	234
Pct. Urban	11%	0%	33%	33%	0%	67%	N/A	0%	0%
Pct. Rural	33%	0%	33%	33%	0%	0%	N/A	0%	0%
Avg. Teacher Assim.	N/A	N/A	46%	N/A	0%	32%	N/A	48%	70%
N Schools	9	3	9	3	1	3	0	4	2
Panel B: Charter Schools									
	2010	2011	2012	2013	2014	2015	2016	2017	2018
Avg. Pct. Female	51%	55%	45%	48%	50%	50%	49%	51%	39%
Avg. Pct. Black or Hispanic	68%	55%	71%	50%	46%	36%	61%	87%	51%
Avg. Pct. Econ. Dis.	75%	66%	77%	75%	73%	52%	82%	86%	79%
Avg. Pct English Learner	0%	7%	5%	4%	7%	1%	20%	1%	3%
Avg. Pct SWDs	12%	12%	11%	14%	12%	11%	15%	9%	17%
Avg. Enrollment	216	255	230	193	228	216	265	448	187
Pct. Urban	50%	60%	57%	48%	33%	50%	25%	50%	0%
Pct. Rural	0%	7%	0%	5%	33%	0%	25%	0%	0%
Avg. Teacher Assim.	N/A	N/A	13%	8%	31%	62%	44%	40%	29%
N Schools	2	15	14	21	6	4	4	2	5

Note. Teacher assimilation is the maximum percentage of teachers who teach at new school *v* in its first year of operation that taught at the same school, *u*, in the prior year. Teacher Assimilation measures not available for 2010-11 and 2011-12 school years and 8 TPS and 9 charter schools that did not report teacher data in their first year of operation. Econ. Dis.=economically disadvantaged. SWD=student with disability.

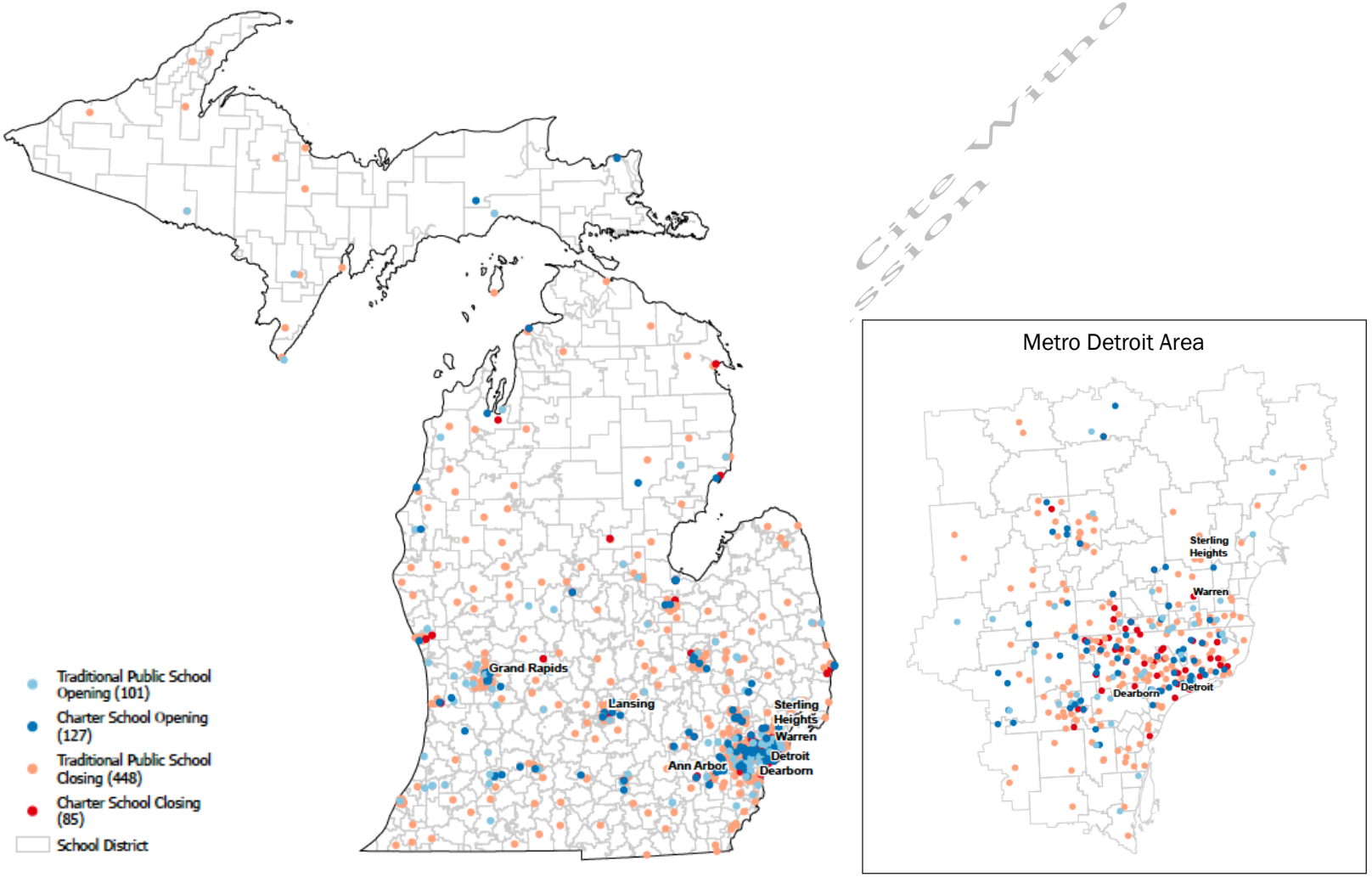
Table 2. Student Characteristics by Sector and Treatment Status

	TPS Sample		Charter Sample	
	Comparison	*Treat*	*Comparison*	*Treat*
Pct. Female	49%	48%	50%	50%
Pct. Black or Hispanic	21%	23%	75%	60%
Pct. Econ. Dis.	49%	49%	82%	73%
Pct. English Learner	5%	7%	9%	11%
Pct. SWDs	12%	12%	11%	11%
Pct. Experienced Closure	2%	37%	4%	5%
Pct. Nonstructural Mover	24%	74%	69%	74%
Avg. Std. Prior Math Score	0.05	-0.07	-0.55	-0.43
Avg. Std. Prior ELA Score	0.05	-0.04	-0.51	-0.40
Pct. Rural School	22%	27%	62%	48%
Pct. Urban School	25%	22%	8%	6%
Avg. Sch. Pct. Female	50%	51%	50%	50%
Avg. Sch. Pct. Black or Hisp.	22%	23%	70%	57%
Avg. Sch. Pct. Econ. Dis.	50%	49%	79%	73%
Avg. Sch. Pct. Els	7%	7%	10%	12%
Avg. Sch. Pct. SWDs	13%	13%	12%	12%
Avg. Sch. Enrollment	455	498	516	537
N Students	950,050	5,655	65,311	5,200

Note. Less than 1% of the sample is missing either prior Math or ELA score. Characteristics are from the year prior to treatment. Non-structural movers are students who attend a different school than the prior year when they were not in the terminal grade of the school they attended the prior year. Econ. Dis.=economically disadvantaged. SWD=student with disability. ELA= English language arts.

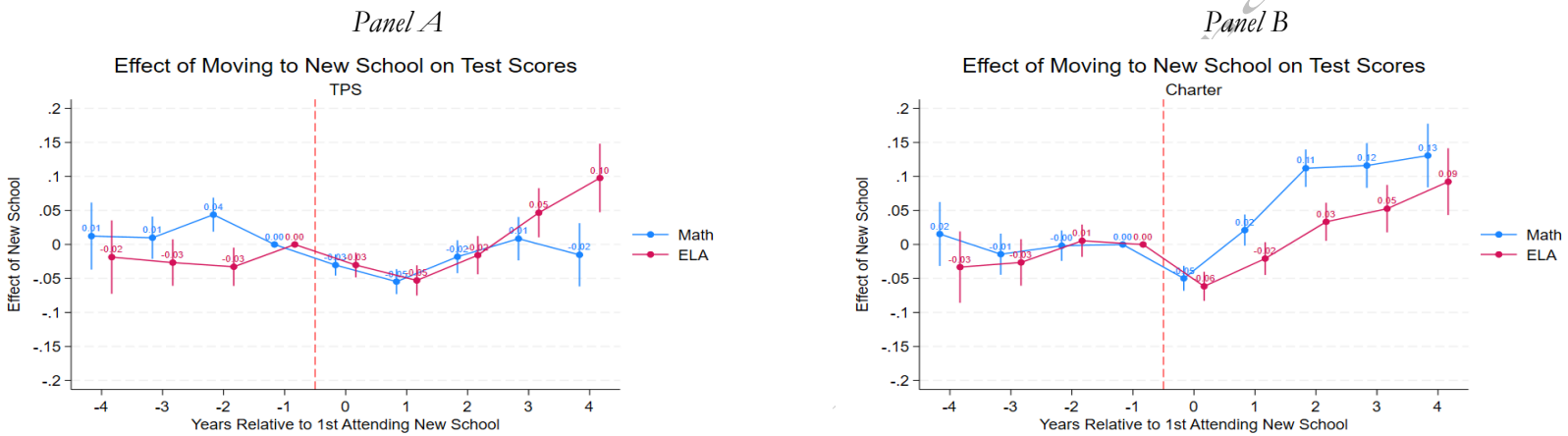
FIGURES AND RESULTS

Figure 1. Map of Michigan School Openings and Closures 2009-2018



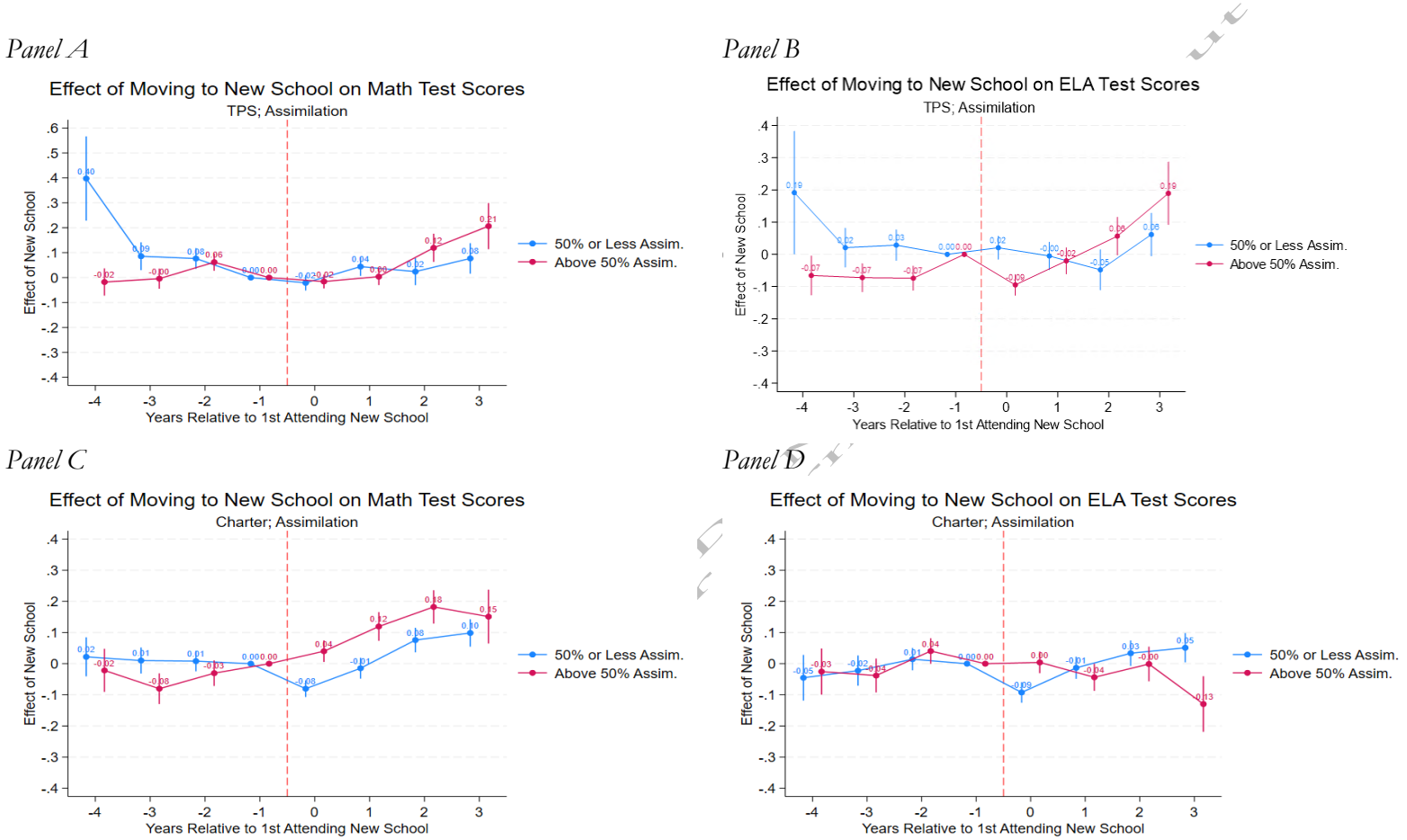
Note. The sample of newly opened schools includes all schools opened after May 31, 2009 and before June 1, 2019. The sample of closed schools includes all schools closed on or after September 1st, 2008 and before September 1st, 2018.

Figure 2. Effects of Attending a Newly Opened School on Student Test Scores



Note. Models include time-variant indicators for gender, race, residential mobility, and economic disadvantaged, English learner, and disability status, the following school characteristics: total enrollment, percent female, Black or Hispanic, English learner and economically disadvantaged students, and the percent of students with disabilities, and student and cohort-year fixed effects. Math sample for Panel A: 4,559,908 student-year observations. ELA sample for Panel A: 4,529,919 student-year observations. Math sample for Panel B: 325,243 student-year observations. ELA sample for Panel B: 325,111 student-year observations.

Figure 3. Effects of Attending a Newly Opened School on Student Test Scores by Teacher Assimilation

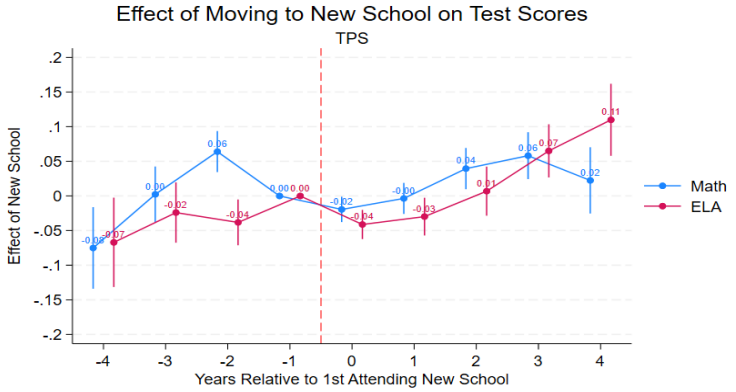


Note. Teacher assimilation is the maximum percentage of teachers who teach at new school *v* in its first year of operation that taught at the same school, *u*, in the prior year. Sample is restricted to the 2012-13 to 2018-19 school years due to teacher data availability. Models include time-variant indicators for gender, race, residential mobility, and economic disadvantaged, English learner, and disability status, the following school characteristics: total enrollment, percent female, Black or Hispanic, English learner and economically disadvantaged students, and the percent of students with disabilities, and student and cohort-year fixed effects. Sample for Panel A: 3,208,109 student-year observations. Sample for Panel B: 3,290,447 student-year observations. Sample for Panel C: 234,782 student-year observations. Sample for Panel D: 234,613 student-year observations.

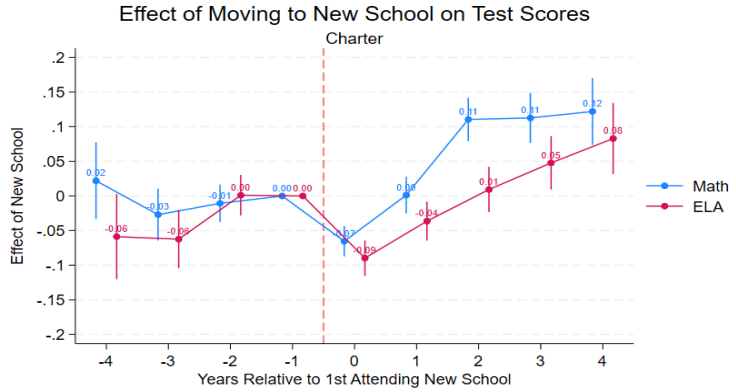
APPENDIX

Figure A1. Effects of Attending a Newly Opened School on Student Test Scores: Non-Structural Movers

Panel A

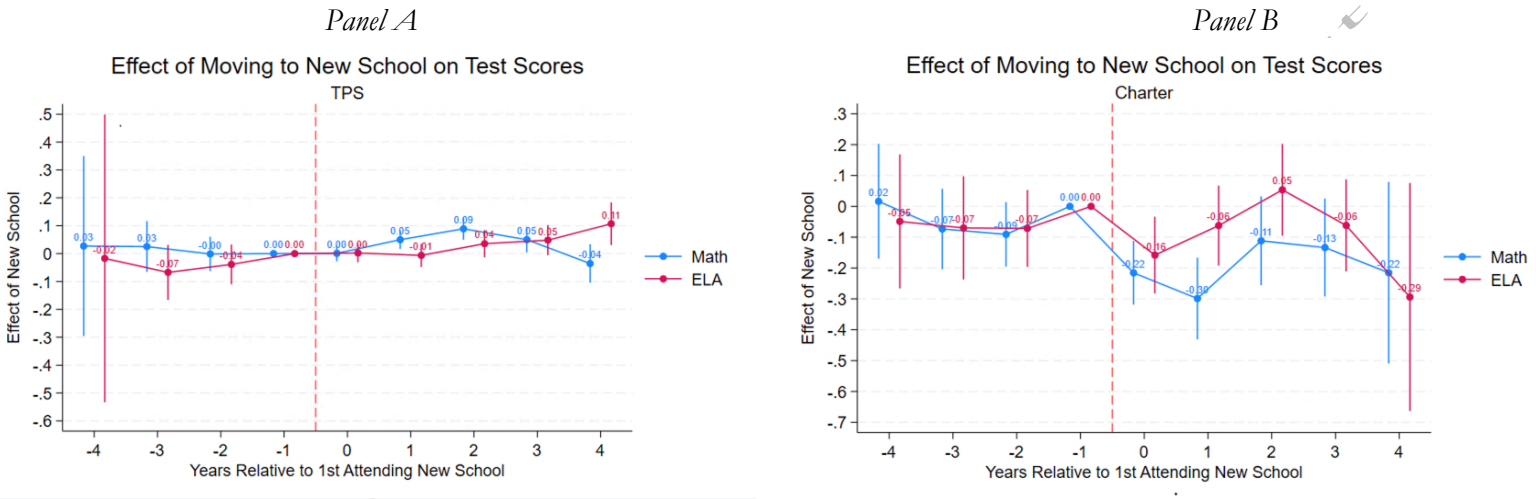


Panel B



Note. Models include time-variant indicators for gender, race, residential mobility, and economic disadvantaged, English learner, and disability status, the following school characteristics: total enrollment, percent female, Black or Hispanic, English learner and economically disadvantaged students, and the percent of students with disabilities, and student and cohort-year fixed effects. Math sample for Panel A: 1,050,239 student-year observations. ELA sample for Panel A: 1,048,549 student-year observations. Math sample for Panel B: 221,655 student-year observations. ELA sample for Panel B: 221,648 student-year observations.

Figure A2. Effects of Attending a Newly Opened School on Student Test Scores: Attended Closed School



Note. Models include time-variant indicators for gender, race, residential mobility, and economic disadvantaged, English learner, and disability status, the following school characteristics: total enrollment, percent female, Black or Hispanic, English learner and economically disadvantaged students, and the percent of students with disabilities, and student and cohort-year fixed effects. Math sample for Panel A: 189,709 student-year observations. ELA sample for Panel A: 189,254 student-year observations. Math sample for Panel B: 23,036 student-year observations. ELA sample for Panel B: 23,048 student-year observations.